

F-4

MAYNORD

**COMMENTS
(W/ REPLY FROM GAINES)**

Comments on Comparison Studies
8 Aug 02
Steve Maynard

Reply #1

General Comments:

1. We need to do everything to reduce the length of this document. I believe the difference plots provide the best info and we should omit CF info to reduce length. There also seems to be a lot of info about truncation. I suggest omit and ref dissertation. I want people to read this document which is need for reducing. Removal of CF info only reduces length by 8-9 pages. Dissertation does not include truncation info presented (see also note below).
2. I think up front we need to define and differentiate between similarity and similarity criteria. I think we are using similarity when we talk about how well the model parameters such as area, width, etc compare to the prototype. Similarity criteria deal with Froude number, distortion, Reynolds number, etc. If we are saying that variability in prototype data affects similarity, or how well we can show our model compares to the prototype, I agree with that concept. If we are saying that prototype variability allows us to relax similarity criteria, such as using greater distortion or Froude number exaggeration, then I completely disagree with that concept and it needs to be placed in individual opinions. You are correct on point 1. Second point is also correct except that our ability to actually determine what prototype Froude number, Shields parameter, etc. (the similarity criteria) are requires some flexibility in applying these criteria. That is not to say that distortion or exaggeration of any parameter is part of the relaxation.
3. I suggest we list the factors that make the comparison of coal bed models and micromodels different. I consider the comparison apples and oranges primarily because the last step in the coal bed model was not variation in vertical scale and datum to match prototype data. The inclusion of the 1973 data is another complication. Although not part of the last step, rail adjustment is, at least in part, an adjustment in vertical scale.
4. We need to differentiate between spatial variability along a reach for a single survey compared to temporal variability at a cross-section between different surveys. Spatial variability is not a big issue because we know that rivers change area, width, etc in pools versus crossings etc. The temporal variability is what makes our life difficult and we must clearly differentiate between the two. I see spatial variability as a bigger issue that you indicate because not all understand the magnitude of changes possible in the MS River from one location to another. Where Prot. variations (crossings vs. bends) are small, one may make the assumption that all rivers behave this way and their definition of reach averages may be different that someone who understands a river that has large variations. See below for temporal variability.

Specific Comments:

1. Page 1-1, line 1- ref to sec 2.4 seems out of place. change to: physical sediment modeling techniques generally rely heavily (or....

2. Page 1-1, 2nd pp- Sentence "If a prototype parameter varies ...". The prototype varies in response to many factors that are not reproduced in the micromodel or most other models as well such as different hydrographs, sediment load, temperature, etc. The model parameter should vary as much as the prototype only if all variables affecting that parameter are reproduced in the model. The last part of this sentence "the similarity criteria should be less stringent" is where I am concerned. If we adopt the above definitions of similarity and similarity criteria and remove the word criteria from the above sentence, then we are in agreement. Remove the word "criteria"
3. Page 1-3- I don't understand the last sentence of the first full pp. The doubt regarding achievement of similarity arises from our inability to measure conditions in the prototype. If prototype conditions can not be accurately determined, similarity has a high degree of uncertainty. Approximations in velocity, discharge, or any other parameter over a model reach results in some measure of flexibility in establishing "scaled" model parameters (even though we can not achieve true similarity because of gravity and viscosity considerations anyway).
4. Page 1-4- What is Gaines (1999)? Description of micromodel procedure prepared for UMR graduate committee fall 1999 by Gaines.
5. Page 1-4, 1st pp- "Based on published literature..." Remove 'the author speculated that the real'. Strike "the author speculated that" and "real"
6. Page 1-4, 2nd pp- ref to Vernon-Harcourt section 2.4.2 needs changing. Change reference to: Freeman (1929) and Ippen (1968)
7. Page 1-5- Since this is a report on micro models, I suggest we replace small scale with micro model. Will consider. I also refer to the WES models as large-scale models. Both measures taken to maintain some semblance of impartiality. Both WES models and micromodel names are used. In general terms large-scale and small-scale are defined by Tables 2-1 and 2-2.
8. Page 1-5- I think it is Struiksma not Strukisma. Correction as noted.
9. Page 1-6- What is this ref to area elev that is in prep? Strike reference -- this is the document referred to.
10. Page 1-7, 2nd pp- I would suggest rewording to something like 'Prototype reaches exhibiting a high degree of variability indicate reaches that are difficult to model while prototype reaches exhibiting a lesser degree of variability are generally easier to model. Will replace last sentence of this para. as suggested.
11. Page 1-7, 2nd sentence- I don't understand this. Values of thalweg position are based on a single cross-section as opposed to sinuosity which is defined over a reach.
12. P 2-1, 1st pp- "The upper ref elev...". Why do we use the word "upper"? Strike the word upper.
13. P2-2, middle pp- Ref to 3.4.2 & 3.43 not found. Change 3.4.2 to 3.1.2 and change 3.43 to 3.1.3.
14. P2-2- What is Maynard et al (2001)? Maynard et.al. refers to the original WES document (blue cover) composed by Steve M., Charlie N., and Doyle ?. Strike the Gaines (in progress) -- that refers to this document. May be more

- appropriate to indicate that descriptions are provided in App. A for the large-scale models.
15. Page 2-7- something wrong with text on Fig 2-2. Correct figure -- text somehow rotated incorrectly in paste operation.
 16. P 2-11, 2nd pp- Last sentence- "The current investigation ...". This is a conclusion that should be put in Conclusions. Move to conclusions in evaluation report.
 17. Page 2-12- What is Gaines (1999)? Need to add Bibliography. Gaines (1999) is a description of the micromodel methodology provided to the UMR graduate committee in fall 1999. The Gaines (1999) reference can be replaced with Max et.al (2002) which was the paper at the EWRI conference -- content is very similar, but the 1999 paper includes a little more detail in procedures.
 18. P 2-13- I think we should omit CF and weighted reach values and ref Gaines Dissertation. The CF portion can be omitted. However, this only saves about 8 or 9 pages. I do not think many with refer to the Dissertation for this, but I think it provides another picture of how well model and prototype morphologic parameters agree.
 19. P 2-19, 3rd pp- "Avg diff values and/or MSE values can ...". Remove "and/or MSE values". Change and/or to and? Either or both of these can be misleading without reference to the range plots. Reference to Fig 2-6 is only for difference. Correct text accordingly.
 20. Page 2-19, "Prototype Variability"- We need to differentiate between spatial variability along a reach for a single survey compared to temporal variability at a cross-section between different surveys. Spatial variability is not an issue because we know that rivers change area, width, etc in pools versus crossings etc. The temporal variability is what makes our life difficult and we must clearly differentiate between the two. Table 2-4 and any discussion of it should be clearly labeled in the former category of spatial variability. Emphasis on spatial variability must be included in the discussion, because it may not be entirely clear of the magnitude of change that occurs between pools, crossings, etc. Some Prot. may exhibit only slight variation while other Prot. may demonstrate large changes in depth/area/width for these locations. The temporal variability is much more difficult, if not impossible, to evaluate. This is partly because of the way we are able to measure the prototype bathymetry. Spatial variability also makes our lives difficult because of its implications on similarity. Friction characteristics and sediment mobility characteristics in a deep bend will be different than in a shallow crossing. While it may be sufficient to assume "reach averages" for larger models, the same may not be true for smaller models esp. with high distortion.
 21. P 2-23, 3rd pp- We should not include other models in statement about limit of +20 LWRP. Change text to: More specifically, MM generally to +20 LWRP. Also need reference after +30 LWRP in sentence following.
 22. Page 2-25 1st pp- Table 2-4 does not show -10. change text to show only +20 and 0 LWRP. Table referenced is on pg. 27 which should be Table 2-5. All following tables and associated references in this chapter (2) should be increased by 1 because of this change.

23. Page 2-28 to 2-43- Too much on truncation- omit and ref dissertation. We had some rule on when we eliminated results due to truncation. Is that rule given here or in Dissertation? Dissertation does not include anything on truncation. It is essential to include this in the comparison report because it is the basis for excluding several models and it impacts what water surface elevation can be used in the calculation of morphologic parameters.
24. P 3-1, 1st pp- Remove sentence "Construction of the Micromodels ...". This is a capability I may not agree with. Use of the Kate-Aubrey reach MM for the evaluation was partly based on the need for micromodeling a continuing nav. problem in the reach. This fact needs to be conveyed with this para. even if the wording is different that shown. Suggestions of wording where you would be comfortable are requested.
25. P 3-7, 1st pp- ref to Section 2.4.3 not found. Also ref to sec 3.3 in next pp. Replace 2.4.2 with Gaines (2002) and 3.3 with 2.1.5.
26. P 3-9, Fig 3-7- We must have blown this figure because bathymetry maps do not show this much difference. For example R-26 on Figure is 2000 ft in error. Not true in bathymetry maps. This echos my thoughts, but the figures are based on data provided by Charlie. Thalweg numbers were recomputed using the modified calculation for thalweg position, but none of the other WES models exhibit this sort of shift. The results have been checked and I've not found anything in the calculation technique that would account for this error. Any suggestions would be welcome.
27. Fig 3-18- The use of 1973 in the micromodel is one of the things that makes this comparison of the models an apple and orange comparison. 1973 was an anomalous event that significantly differs from the LRLR sequence agreed to in Rolla. The LRLR sequence is key in determining state of MM calibration. However, reach dynamics are in large part driven by conditions preceding the 1975/1976 timeframe. I am not sure that the 1:300 model should not have considered conditions that led up to the problem (LRLR crossing). How far should the present discussion go?
28. P 3-36- Reach plots rather than CF needed for plan conditions. Both reach plots and CF plots provided. Tables preceding pg. 3-36 refer to both reach (Table 3-3 & 3-5) and CF (Tables 3-4 & 3-6) plots. I am unclear what this comment refers to on pg. 3-36?
29. P 3-35- I suggest we start with a typical evaluation of the plan maps and assess the quality of the verification. I found little similarity in the model and prototype plan conditions. One major diff was there was no nav channel in model. Calibration of MM discussed in preceding section (3.1.3). Verification of WES discussed 3.1.2. Is more discription suggested by this comment? Predictive cases discussion can also be expanded, is this what comment refers to? I think inclusion of dredging information may help in interpretation of MM results when compared to the Prot.
30. Table 3-9,3-10- Use arithmetic mean and omit CF and weighted. Any real significance to omitting the other two methods? The values provide a sense of how reach parameters would vary by different calculation methods.
31. P 3-36- ref to sec 3.4.2 not found Should be 3.1.2.

32. P 3-47, 1st pp- ref to section 3.3.5.1? Should refer to Gaines (2002)
33. P 3-47, last sent of 1st pp- replace case studies with two Kate Aubrey models.
Change as noted.
34. P 3-47- I don't understand last pp. This p. discusses the fact that although the numerical value of differences between the 1:300, 1:8000, and 1:16000 models are about the same (in a relative sense), that the two larger models under predict area by that margin while the smallest model over predicts area. The over prediction of area leads to greater differences in flow and velocity distribution as the section is narrower and deeper than it should be. Strike Paragraph.
35. Table 3-10- Thalweg position is from an arbitrary line which makes % meaningless. Had we chosen the arbitrary line much further from the channel, the % would have been much less. Correct for the 1:300 model; however, the numbers as expressed for the 1:8000 and 1:16000 models provide a relative comparison between the two (they are both based on the same reference point). The thalweg index method used in earlier analysis alleviated this issue for all three models but wasn't used for the numbers in this table.
36. P 4-1- List factors that make comparison of coal bed and micromodel difficult. An all inclusive list is not possible. Do you suggest major items such as: verification process versus calibration process, sediment feed vs. sediment recirculation, rail adjustment versus selection of vertical scale, some similarity in friction characteristics versus no consideration of friction, some efforts to use a discharge scale vs. no attempt to scale discharge?
37. P 4-12- I would prefer to leave conclusions out and put in our main report. This can be done if team consensus to do so.
38. P 54, Figure B-7.16- backwards Will make necessary correction.
39. P 53 Fig B-7.1a- range lines / numbers are not correct- all figures show 34 ranges. Figures B-7.2b-e are incorrect (they are for the "predictive" case -- 1997). These figures will be corrected to show data for the 1994 comparison which is ~Ranges 2-25.
40. Description of Range data - need to emphasize Range Plots over length of model as best